OUR ASTRONOMICAL COLUMN

SOLAR PARALLAX FROM OBSERVATIONS OF MARS.—In an appendix to the Washington Observations for 1877, Prof. Eastman, of the U.S. Naval Observatory, deduces "a value of the solar parallax from meridian observations of Mars at the opposition in 1877." In September, 1876, a circular was addressed from Washington to the principal observatories in both hemispheres, inviting co-operation in systematic meridian observations of Mars at the close opposition of the following year, and in response series were received from the Cape of Good Hope, Melbourne, Sydney, Cambridge, U.S., Leyden, Kremsmunster, and San Fernando, but Prof. Eastman excludes from his investigations the observations at the last two observatories, in the absence of sufficient details as to the methods and instruments employed. In the circular it was proposed to follow virtually the method of observation adopted at Pulkowa, by Prof. Winnecke in 1862, but it is stated, "The prescribed method of observing was *fully* carried out at only *two* stations and *partially* at one. Where the plan of the circular was strictly followed, the character of the work was decidedly superior to that where the directions were disregarded."

The results of the comparisons are thus given :—

	Sun's Parallax.	No. of Comparisons.
Washington and Melbourne	8˙971	19
Washington and Sydney	8˙885	7
Washington and Cape of Good Hope	8˙896	7
Melbourne and Leyden	8˙969	27
Melbourne and Cambridge, U.S.	9˙138	10

With respect to the large value of parallax given by comparison of Melbourne and Cambridge, Prof. Eastman remarks : "This difference may arise from the method of observing over inclined threads at Cambridge, for the agreement of the results among themselves is very satisfactory ; but, whatever the cause of the discrepancy may be, it has not been deemed advisable to employ these values in obtaining the final result."

The mean of the remaining sixty results, with regard to the computed weights, gives for the solar parallax, 8″˙953 ± 0‴˙019.

It has been assumed that this method of determining the sun's parallax is certain to give too large a value, and Mr. David Gill, now H.M. Astronomer at the Cape, has suggested a definite cause ; but Prof. Eastman, after experimenting upon Jupiter, does not find in his case that Mr. Gill's theory holds good. He intends, however, to pursue the investigation upon the disk of Mars.

VARIABLE STARS.—An ephemeris of the variable stars, similar to those of previous years, has been issued by the "Astronomische Gesellschaft" for 1882. It contains the times of maxima and minima of most of the variables whose periods are known, including, in addition to Algol, five stars of the Algol-type, viz. λ Tauri, S Cancri, δ Libræ, U Coronæ, and U Cephei. A minimum of Mira Ceti is fixed to February 3—this phase has been much less observed than the maximum. Both this minimum and the following maximum on May 23 are dated about ten days earlier than Argelander's formula of sines would indicate, but the observations of the last ten years have shown additional perturbation. A minimum of χ Cygni is dated February 20, and a maximum on August 25. The following are Greenwich times of minima of Algol :—

h. m.	h. m.	h. m.
Feb. 1, 8 28	March 10, 15 4	April 2, 13 35
15, 16 33	13, 11 53	5, 10 24
18, 13 22	16, 8 42	22, 15 17
21, 10 10		25, 12 6
24, 6 59		28, 8 55

Minima of S Cancri occur February 16 at 11h. 23m., March 7 at 10h. 38m., March 26 at 9h. 54m., and April 14 at 9h. 9m.

For U Cephei (Ceraski's variable) calculated times of minima are :—

h. m.	h. m.	h. m.
Feb. 1, 15 8	March 3, 13 3	April 2, 10 58
6, 14 47	8, 12 42	7, 10 38
11, 14 26	13, 12 22	12, 10 17
16, 14 6	18, 12 1	17, 9 56
21, 13 45	23, 11 40	22, 9 35
26, 13 24	28, 11 19	27, 9 15

A minimum of U Coronæ is dated February 6 at 10h. 7m. ; the period is 3d. 10h. 51˙24m. ; the extent of variation about one magnitude.

THE ROYAL ASTRONOMICAL SOCIETY.—We are happy to be able, on the authority of Prof. Winnecke, to correct a mis-statement in this column, referring to the decease of M. Gautier as leaving Prof. Plantamour the senior Associate on the list of this Society. Notwithstanding some reports to the contrary, Prof. Winnecke informs us that this position is occupied by Prof. Rosenberger, who is still alive and in good health. Forty-five years have elapsed since the Society's gold medal was presented to Prof. Rosenberger, at the hands of Sir George Airy, for his masterly and elaborate researches on the motion of Halley's Comet. He was elected an Associate in April, 1835.

THE GREAT COMET OF 1881.—On January 7 and 8 Prof. Winnecke obtained good determinations of the position of this comet, which is still well observable with the great refractor at Strasburg. Its apparent diameter was about 30″, and there was a condensation presenting the brightness of a star of 13½ m. The resulting places are—

	M.T. at Strasburg.		R.A.		Decl.
	h. m. s.		h. m. s.		° ′ ″
January 7	7 49 6		22 50 21˙70		+57 48 59˙0
8	7 33 3		22 52 49˙72		+57 42 15˙2

It will be seen that Dr. Dunér's ephemeris in the *Astronomische Nachrichten* still gives the comet's position pretty closely.

THE DETERMINATION OF ELECTROMOTIVE FORCE IN ABSOLUTE ELECTROSTATIC MEASURE

HAVING already described my absolute sine electrometer before the Physical Society and at this year's meeting of the British Association, there is no necessity for describing here more than the prominent features of the instrument. Two plates of brass, each about one foot square, their surfaces being rendered true planes, are connected together, as a rigid body, by four ivory axes passing through both plates near their corners. On these axes are placed (between the plates) washers of mica, which serve to keep the plates asunder and parallel at a very small distance from each other. One of the plates is continuous ; the other (the guard plate) has in its centre a square aperture whose side is 3 centimetres long, and in this aperture hangs a very light disk of aluminium suspended from the top of the guard plate by two Wollaston platinum wires each about 7˙5 inches long. The disk is flush with the guard plate when it rests against four fine screws attached to the latter. The system of plates is movable, as a rigid body, round a horizontal axis, and its motion is produced by a micrometer screw (1-16th of an inch pitch) working against an insulated portion of the lower edge of the continuous plate ; thus the screw tilts the system out of the vertical to a measurable amount. The horizontal axis of the plates is carefully levelled with a cathetometer, and the exact distance between the plates is determined by three readings of a spherometer taken at the aperture of the guard plate (previous to the insertion of the disk) before the mica washers are inserted between the plates (the plates being in complete contact), and three readings at the same points after the insertion of the washers. The vertical distance between the centre of the axis of plates and the point of the micrometer screw is 15 inches ; the weight of the disk ˙2568 grammes ; and the head of the micrometer screw is a circle 3 inches in diameter, divided into 1000 equal parts.

The essential principle of the instrument will be understood from the following figure. B is the horizontal axis about which the plates C (the continuous plate) and G (the guard plate) are tilted by the fixed micrometer screw A. The disk is represented by the full line D in the centre of the guard plate.

To measure the E.M.F. of a battery, put C in connection with the positive pole, while the negative pole and the guard plate (and, with it, the disk) are connected with earth.

If N is the attraction exerted on the disk by the charge on C, W = weight of disk, θ = angle of deflection of the plates from the zero, or vertical, position, we shall have, when the disk is *just* out of contact with the little screws which keep it flush with the guard plate,

$$N = W \sin \theta \quad \cdots \cdots \cdots (1)$$

For the particular instrument which has been constructed for

me by Mr. Groves of Bolsover Street, the above equation becomes (using the circular measure for the sine)

$$V = \cdot 0456 \sqrt{m} \quad . \quad . \quad . \quad . \quad . \quad (2)$$

in which V is the difference of potential between the poles of the battery, and m is the number of turns of the micrometer screw from the vertical position (in which $\theta = 0$) to the limiting position of equilibrium, or that in which the disk is *just sustained* out of contact with the screws.

The disk is observed from behind the guard plate by means of a microscope attached rigidly to the plate and moving, of course,
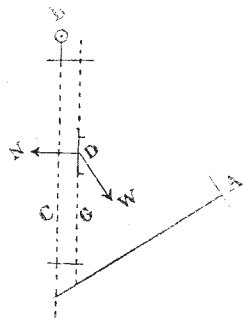
with it. The slightest motion of the disk can be thus seen; and when, by tilting the plates, the attraction N ceases to cause a motion of the disk, we know that the limiting inclination θ is attained.

As there would be great difficulty in determining the vertical position of the plates accurately, I do not seek to determine it, but use a differential method. Thus, suppose that we use n cells in each of which the E.M.F. is E, and let the reading of the micrometer be m when the limiting position is reached; now use n' of the cells, and let the reading for equilibrium be m'; then we know $m - m'$ but not m and m' separately. Substituting nE for V in equation (2), we have

$$n^2 E^2 = (\cdot 0456)^2 m.$$

Also

$$n'^2 E^2 = (\cdot 0456)^2 m'.$$

$$\therefore \ E = \cdot 0456 \sqrt{\dfrac{\Delta}{n^2 - n'^2}}, \quad . \quad . \quad . \quad (3)$$

where Δ stands for $m - m'$.

Of course it is obvious that, to reduce any error of reading to a minimum, it is advisable to use for n and n' a large number and a small number, respectively. With the present instrument it is not possible to use more than about fifty Leclanché cells, because these produce such a large displacement of the disk that the amount of play allowed is exhausted.

A series of experiments carried out last summer in Prof. G. Carey Foster's laboratory gave for the E.M.F. of each cell of a battery of fifty Leclanchés ·00475 absolute electrostatic units.

This was obtained on the supposition that the E.M.F. was the same in all the cells, a supposition which is extremely improbable.

Within the last few days I have repeated the experiments on a different principle. My first idea was to work with a battery of Grove elements. Each element consisted of a test-tube containing a saturated solution of sulphate of zinc, and inside this a smaller test tube containing nitric acid, both test-tubes having the same axis, and both fitted into a paraffin cork, or stopper. A little zinc rod (surrounded with a very thin glass tube open at its lower end) plunged into the liquid of the outer test-tube and came up through the paraffin stopper; a platinum wire came up similarly out of the nitric acid; and electrical communication between the liquids was maintained by an aperture in the inner test-tube, through which the fumes of the nitric acid passed into the outer.

The resistance of the cells was, of course, enormous. They were formed into a battery, and supported in a wooden board soaked with paraffin.

The result then obtained for the E.M.F. of the Grove was much below what I knew to be about its value. The reason of this appeared to be that, with the great internal resistance of the battery, the external resistance was not sufficiently great. I

therefore *diminished the internal*, and at the same time *increased the external*, resistance by inserting threads of asbestos through the apertures in the inner test-tubes, the extremities of every thread dipping into both liquids, and also by suspending each cell separately by a fine silk thread, about 2 feet long, from a fixed horizontal glass rod. The result was an increased, but still unsatisfactory, value of the E.M.F., and the unsatisfactory result was due to the fact that the nitric acid gradually attacked the zinc rods.

The employment of cells of exceedingly high resistance for the measurement of electromotive force is open to the serious objection that with them it is necessary to have a practically infinite external resistance, and this it is not always easy to attain. Even with the Thomson quadrant electrometer such cells give an uncertain result. When we have to trust for conduction to fumes or a moist film between two glass vessels containing the liquids, we occasionally get no indication whatever from the electrometer, and it is only by shaking up the cells that the requisite conductivity is obtained.

The above form of battery was abandoned for a series of chloride of zinc elements. Here the internal resistance is comparatively small, but we must not assume all the cells to have the same E.M.F. I therefore took forty of these elements, and compared their electromotive forces by a Thomson quadrant electrometer. In this way I found a variation of more than 8 per cent. in the E.M.F. of two cells.

Denote the electromotive forces of the cells by E_1, E_2, E_3, ... and let D stand for the electromotive force of a given Daniell, or any other element whose E.M.F. is to be found absolutely; and let the *ratios* of E_1, E_2, . . . to D, as determined by a Thomson quadrant electrometer, be r_1, r_2, r_3. . . . .

Now suppose that we use any number of the cells with the absolute sine electrometer, and that the sum of their electromotive forces, $(r_1 + r_2 + r_3 + ...)$D, is denoted by $D.\Sigma r$. Note the reading of the micrometer screw when the limiting deflection of the plates is reached. Then use a smaller number, whose total E.M.F. is represented by $D.\sigma r$, and take the new reading. If Δ is the difference of readings, we have by equation (3)

$$D = \cdot 0456 \sqrt{\dfrac{\Delta}{(\Sigma r)^2 - (\sigma r)^2}} \quad . \quad . \quad . \quad (4)$$

The ratios r_1, r_2, . . . must, of course, be marked on pieces of paper attached to the outsides of the corresponding cells.

I quote the result of the measurement of the E.M.F. of a particular Daniell. Taking observations with 39 of the chloride of zinc cells and with 10 of them I found

$$\Delta = 14\cdot 2 ;$$

also the registered values of the ratios r_1, r_2, . . . gave in this case

$$\Sigma r = 50\cdot 427D ; \ \sigma r = 12\cdot 834D ;$$

and by substituting in equation (4) we have

$$D = \cdot 00352 \quad . \quad . \quad . \quad . \quad . \quad (a)$$

absolute electrostatic units as the E.M.F. of the Daniell.

Again, taking observations with 39 and 20 of the cells, I found

$$\Delta = 11\cdot 17 ;$$
$$\Sigma r = 50\cdot 427D ; \ \sigma r = 25\cdot 918,$$

and these numbers substituted in (4) give *exactly* the value (a) above.

It is not possible, with the present instrument, to work with two batches of cells differing slightly in number; for I find that in some cases I cannot be certain of the reading corresponding to limiting equilibrium within about one-fifth of a revolution of the screw head. This uncertainty is of no consequence when Δ is large; but it is capable, I believe, of being almost completely got rid of.

Sir William Thomson's final estimate of the E.M.F. of a Daniell is

$$\cdot 00374$$

absolute electrostatic units ("Electrostatics and Magnetism," p. 246).

The Daniell cell used in the above experiments was a particular form of "gravity" arrangement, and I have good reason to believe that its E.M.F. was somewhat below that of a normal Daniell. Hence the value obtained for its E.M.F. may be quite consistent with Sir William Thomson's number.

I hope before long to determine by means of the absolute sine electrometer the E.M.F. of a cell which is also known in electro-

magnetic measure, on account of the supreme importance of such a measurement in the theory of light.

I may in conclusion refer to a possible objection. The force of "stiction" may be supposed to interfere with the reading of the limiting position of equilibrium. Practically the objection is groundless, for we can always (force of stiction notwithstanding) attain this position very nearly. Having done so, a very slight tap on the base of the instrument is sufficient to free the disk and take it slightly out of focus, where it remains. Then move the plates forward by means of the micrometer screw until the guard plate again catches up the disk. We thus get the position of equilibrium without the interference of stiction at all.

I am now having the instrument altered by Mr. Groves. A very light and flat gilt disk of mica suspended by silvered silk fibres will replace the aluminium disk, and the distance between the plates will be varied within very narrow limits, so as to show whether the cushion of air between the plates exercises any influence on the results.

The range of tilting of the plates will also be increased so as to allow of the employment of a large number of cells. In this case the equations previously used must be replaced by equations of the forms—

$$\text{Sin } (\theta + a) = k E^2,$$
$$\text{Sin } a = k' E^2,$$
$$\tan (\theta + a) - \tan a = c,$$

where k, k', and c are known constants. In these equations we can, of course, take a very small, as before, and get a very approximate and easily obtained solution by using expansions to the third order of small quantities—as I shall show in a subsequent communication on the completion of my experiments.

GEORGE M. MINCHIN
Royal Indian Engineering College, Cooper's Hill,
December 1

VELOCITIES IN TIDAL RIVERS

A PAPER "On the Relative Value of Tidal and Upland Waters in maintaining Rivers, &c.," by Mr. Walter R. Browne, M. Inst. C.E., late Fellow of Trinity College, Cambridge, has been lately published by the Institution of Civil Engineers. The main object of it is to prove, as a general principle, though by no means applicable in every case, that the main agent which keeps clear the channels of tidal rivers is not the run of tide passing up and down them every twelve hours, but the upland or fresh waters which pass down them at the period of low water, more or less aided by the oozing out of salt water which has soaked into the banks while covered with the tide.

The author, with a view to check his conclusions by actual experiment, resolved to investigate the actual velocity at the bottom of a tidal channel during an ebb tide ; since it is clear that, whatever the velocity at the top, it is the bottom velocity alone which produces any scouring effect. In the very largest rivers, above the action of the tide, the bottom velocity differs but little from the surface velocity ; but in smaller streams it is generally much less than the surface velocity, and the ratio between the two decreases rapidly as the depth increases. The case of a tidal river however is somewhat different, because then the level of the bottom is below the surface of the ocean outside, and this must have a certain effect in ponding back the river current. Accordingly two sets of experiments, made at a carefully-chosen spot on the River Avon at Bristol, showed that for about two-thirds of an ebb tide, and even when the surface velocity was at its highest, the bottom velocity was absolutely *nil*. The water at the bottom then seemed to start suddenly into activity, and almost immediately assumed a velocity agreeing fairly with that observed in ordinary rivers above the tidal area. The two sets of experiments were made with different meters, at different states of the tides, and at different times of year ; so that they amply confirmed each other. The stillness of the bottom was further proved by the board, supporting the rod on which the meter was hung, coming up with a deposit of silt upon its surface, showing that, far from any scouring being in progress, actual deposition was taking place. The second set of experiments was the most accurate, the meter having been specially made and tested for the purpose. The results are plotted on the accompanying diagram.

In the diagram the line A B represents the probable variation in the surface velocities as sketched from the various observations. The small circles represent the observations made at

different times by the meter, and the crosses represent observations made, as a check upon these, by floats at the surface. It will be seen that the meter observations are by far the most satisfactory. The line C D is similarly sketched from the observations of the velocities at the middle of the depth. It will be observed that here the maximum velocity is attained later than at the surface, and just when the latter is beginning to fall off. The line E F G and the circles contiguous to it refer to the bottom velocity. It will be seen that it rises from nothing to a tolerably high value, with very great abruptness, just at the time when the surface velocity begins to diminish : it is probable, however, that
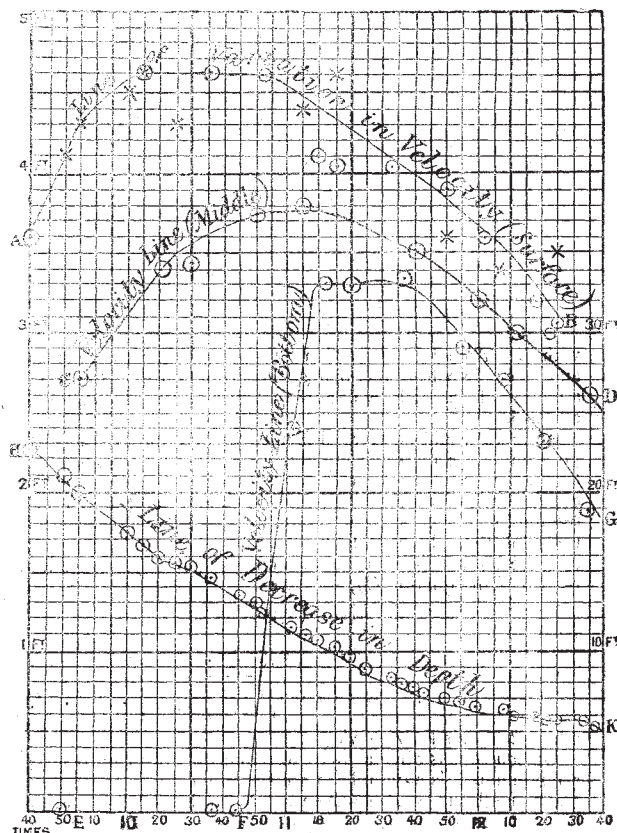
the change is not so much connected with this as with the decrease in depth, which is given by the line H K and the contiguous circles. These are plotted to one-tenth the scale of the velocity observations, and the sudden flattening of the curves about 12 noon marks the beginning of what the author terms the low water period.

It is believed that the fact of a current having a high velocity at the surface and absolutely none at the bottom has not been previously observed, and it may have considerable bearing on the general theory of the motion of rivers, as well as on the more practical points dealt with in the paper.

ON THE ECONOMICAL USE OF GAS-ENGINES FOR THE PRODUCTION OF ELECTRICITY[1]

THE lecturer pointed out, that as long as the chief practical use of electricity was in telegraphy it was the quickness of action, rather than the ability to transmit large amounts of power to a distance, that formed the chief feature in the employment of electricity ; but that in this exhibition the numerous practical examples of the electric transmission of power, rather than the electric transmission of signals, formed without doubt the leading feature.

Much had been heard about the dynamo-electric machines which generate the electric current ; but while electricians were

[1] Abstract of a lecture delivered in French in the Salle du Congrès, at the Electrical Exhibition, Paris, by Prof. W. E. Ayrton, F.R S.